

## The authorship of the name *Amytornis textilis*: a reply to Black, Schodde & Préviate

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In their splendid detective work relating specimens of grasswrens to geographical origins, Black *et al.* (2013) have resolved the primary questions that they faced, but we consider that they have erred in seeking to ascribe authorship of the name *Malurus textilis* (Western Grasswren) to Quoy and Gaimard.

They correctly point out that the *Dictionnaire des sciences naturelles* appeared some months before the text or the plate published by Quoy and Gaimard (1824), the text of which, and probably the plate, was in livraison 3 of their volume, which has been dated 28 August 1824 (Zimmer 1926). They argue that Dumont 'worked closely' with these authors and that he used their information; and indeed Dumont explicitly mentioned Quoy & Gaimard. However, this must have been many months earlier because Dumont must have finished his part of the dictionary before leaving Toulon on 11 August 1822 on the frigate *La Coquille*, on which he only returned to France on 24 March 1825 (Simpson 2012). However, he subsequently took command of the *Astrolabe* on which Quoy and Gaimard served on a four-year voyage of discovery (Quoy & Gaimard 1830). In 1822 it is possible that he thought their work would appear first, but it is interesting to see how long each account took to appear.

However, Dumont's reference to these authors is typical of a reference to a manuscript name of a kind common at the time, when in all likelihood it was the author's intention that the credit should be attributed to those who coined the name. It is apparently argued that the description uses the very same wording as appears in Quoy and Gaimard's later text. That is not contested, but if we are to accept this change we would find ourselves having to accept many others that are usually seen as manuscript names.

For a somewhat parallel but more complex case see Dickinson (2003): a manuscript submitted to Thomas Horsfield by John McClelland contained a variety of names, wherein the description in English is no doubt that of McClelland, while those in Latin were almost certainly added by Horsfield, who became the paper's author when he presented it to the Zoological Society of London. Many other cases could be cited and we personally find the use of quotation marks important internal evidence that the description is indeed from another person said to have written it. We concede that others may well find this view too narrow, but in the case in question the decision does not rest on the presence of absence of such punctuation.

The issue turns on the interpretation of Arts. 50.1 and 50.1.1 of the *International code of zoological nomenclature* (ICZN 1999). Art 50.1 begins 'if it is clear from the contents ...' and Art. 50.1.1 reads 'However, if it is clear from the contents that some person other than an author of the work is alone responsible both for the name or act and for satisfying the criteria of availability other than actual publication, then that other person is the author of the name or act.'

Our first impression, based on the information given by Black *et al.* (2013), was that Dumont had himself contributed to the description because on p. 118 Dumont states 'On voit au Muséum de Paris un individu de cette espèce dont la mandibule supérieure est très-aiguë et recourbée à sa pointe, et un autre dont le plumage est d'une couleur plus foncée'.

However, we have been assured that these very words also appear in the later publication by Quoy & Gaimard. Thus we accept that an available manuscript by Quoy & Gaimard must be seen as the source of the entire description.

The Code usually appears to ask that we rely on the very specific evidence of the original work. For example, in Art. 32.5, it states 'if there is in the original publication itself, without recourse to any external evidence, clear evidence...'. In Art. 50.1 we find 'from the contents'. It is possible to interpret this different choice of words to deliberately mean something different or to mean the same. However, if it were intended that 'external evidence' be acceptable we would want the Code to say so.

In this case it is not disputed that the external evidence was published later. Thus it seems to us that one must accept that unfortunately and accidentally Dumont 'usurped' the role of Quoy & Gaimard and that *Amytornis textilis* and *A. leucopterus* must continue to be attributed to Dumont.

Lafresnaye (1842) described *Grallaria squamigera* (Undulated Antpitta) 'Florent-Prévost, *Zool. du voy. de la Vénus*, pl. 2 and *G. guatemalensis* (Scaled Antpitta) also from that source. No evidence is known to us of the publication of these plates until later (*cf.* Dickinson *et al.* 2011), but the plates bear both scientific and French vernacular names, and it would appear that Lafresnaye must have seen them. Both names are credited to Lafresnaye on the grounds of precedence. We see this as no different from the supposed 'usurpation' of the role of Quoy & Gaimard by Dumont. And, let it be clear, these were in no way mean-spirited actions; in both cases the authors we have been crediting stated who they believed the authors to be and no doubt intended that they be credited. If zoologists do not assign that credit where it belongs this is down to the wording of the Code. Thus the 'usurpation' to which we refer was not by Dumont or by Lafresnaye, rather it flows from the discipline imposed by the Code. We would venture to suggest that there are probably well in excess of 100 species-group names in ornithology that could be justly reassigned, but we do not think we should do so without the explicit support of a less ambiguous Code.

We hope that Black *et al.* (2013) will agree that the wording of this Article in the Code could usefully be revised to remove the ambiguity that permits us to reach different conclusions. In the context of this note, we would further suggest that consideration be given to whether it is right to exclude from any credit the joint author of a name in a paper published by one author, an exclusion which the Code as written seemingly effects. For an example of this see Blyth (1861) where one genus-group name and eight species-group names were proposed. In the light of modern practice where entire multi-disciplinary authorship teams gain credit for new names it is unreasonable to deny an author the right to formally include a colleague as a fellow author of the descriptions; nevertheless, we agree with the wording of Recommendation 50A in the Code and we do not suggest that finding a clearer formulation of Art. 50 will be easy.

In this context we draw attention to the suggestion by Dubois (2008) that citing dates for names should suffice. Even when that is done there may still be some concern over their accuracy. Confusingly, we already have a minivet name *Pericrocotus speciosus fokhiensis* proposed twice, once in 1910 by Buturlin and in 1920, with a quite different type and type locality, by Baker (*cf.* Dickinson *et al.* 2002).

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